

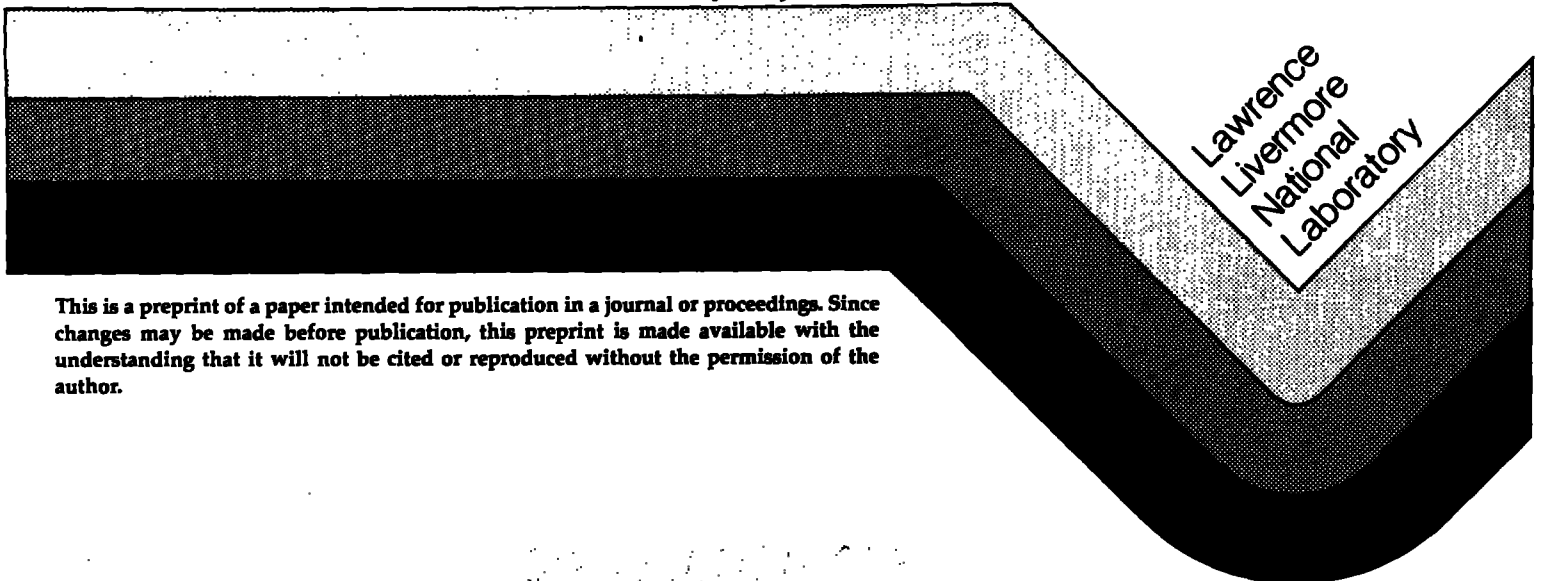
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CONTROVERSIES IN THE GEOLOGICAL SCIENCES:  
The Conduct of Investigations

R. N. Schock

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Glass in Planetary and Geological Phenomena  
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# HISTORICAL CONTROVERSIES IN THE GEOLOGICAL SCIENCES\*

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## ABSTRACT

The dispute over the origin of tektites, either in its scope or in the ferocity of the arguments, is typical of disputes which have characterized the geological sciences for at least 250 years. In view of the impossibility of reconstructing the events which created the debated result, it is perhaps surprising that even more controversies have not erupted. A review of the literature of these controversies suggests several points to consider that might if taken facilitate the resolution of the dispute with benefit to the progress of science. The first of these is to contemplate your arguments in the context of those of your detractors, not solely in terms of your own ideas. Make careful field observations, not only laboratory observations and theoretical arguments; the record of the natural event is in the field, not the office or the laboratory. Search for outrageous hypotheses and then test and refine them to see if some new insight is gained. Finally, look for the simplest explanation; it is likely to be elegant despite its dress and it probably will be correct.

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## INTRODUCTION

When asked if I would consider giving the banquet address at this conference, I reflected back to the most important class that I took as an undergraduate. That class was an elective 2-hr unit called simply, Geologic Literature. It wasn't The Geologic Literature, A Study of Geologic Literature, or Readings in Geologic Literature, just Geologic Literature. I learned more from that one class than any other single undergraduate class because we were given an opportunity to study how science, and in particular the science of natural phenomena which have already taken place, is actually conducted. I'm sure that like me, many of you were fascinated more by James Watson's book<sup>1</sup> The Double Helix, than by the discovery of the structure of DNA itself, despite all that the latter portended. The reason for this is that we all know, as scientists, that the real story is as much how science is actually accomplished, as what in fact is discovered. That is not to belittle for one minute the discovery, because that is after all our goal. Yet there are many lessons to be learned in studying how science is really accomplished and these lessons can be helpful to all of us as we pursue our own careers. This is perhaps no more true anywhere than in the earth sciences, where nature's experiments cannot be repeated for verification, and therefore arguments cannot be easily silenced.

To be sure, there are controversies in all sciences. However, I would argue that in the other physical sciences they are fundamentally different than those in the earth sciences for the very reason that I have just mentioned; the experiment is usually on such a grand scale and involves so many parameters which are unknown, that it is simply impossible to model the

original event, and no one was there to record their observations. The best that we can do is to carefully study the results of these events and then attempt to reconstruct the happenings themselves, either in the laboratory, on paper, or in our minds. Controversies in physics, by contrast, tend to be of a different gender. For example, in 1900 Boltzmann passionately argued that the motion of atoms was random and that a statistical treatment was the only way to approach a unified theory. When he failed to convince the purveyors of the prevailing view (there was never really any argument, Boltzmann was mostly ignored), he became despondent and in 1906 committed suicide. Boltzmann however, in addition to being correct, was impatient. It was only a matter of months after his death before the elegance and veracity of his treatment became almost totally accepted. In contrast, the earth sciences are riddled with controversies that have grown into feuds and then full-scale battles, over decades and even centuries, because there is always plenty of room for doubt. Of course, the correct view eventually prevails, but often with much human suffering, needless antagonism, and probably to the detriment of the speed at which the science is accomplished, if not to the science itself.

The argument that is central to this conference, about whether tektites and other glasses are terrestrial or lunar, and meteoritic or volcanic, in their origin has raged for years and I do not propose to add to the debate. I rather thought it would be both entertaining and instructive to examine a few other controversies that have characterized the geological sciences in the light of their subsequent resolution. In doing this, it becomes immediately apparent that individual personalities play a major role, and one can almost perform psychoanalysis on historic figures, by the way in which they presented their arguments, and the persistence which they showed, right or wrong.

Before beginning, it is worth remembering the words of T. C. Chamberlain, who in his now classic paper<sup>2</sup> on the method of multiple working hypotheses written almost 100 years ago, wrote that in the tentative stages of a hypothesis,

"...the affections enter with their blinding influence. Love was long since discerned to be blind and what is true in the personal realm is measurably true in the intellectual realm. Important as the intellectual affections are in stimuli and as rewards, they are nevertheless dangerous factors in research. All too often they put under strain the integrity of the intellectual processes....While he persuades himself that he holds it still as tentative, it is none the less lovingly tentative and not impartially and indifferently tentative. So soon as this parental affection takes possession of the mind, there is apt to be a rapid passage to the unreserved adoption of the theory....The mind lingers with pleasure upon the facts that fall happily into the embrace of the theory, and feels a natural coldness toward those that assume a refractory attitude. Instinctively there is a special searching-out of phenomena that support it, for the mind is led by its desires....When these biasing tendencies set in, the mind rapidly degenerates into the partiality of paternalism....The theory then rapidly rises to a position of control in the processes of the mind and observation, induction and interpretation are guided by it. From an unduly favored child it readily grows to be a master and leads its author whithersoever it will."

In the examples to come, I believe that we will see ample evidence of loving yet blind embraces.

I have often felt that one of the more humorous incidents in the geological literature is the work of Sedgewick and Murchison. Adam Sedgewick, a Welsh geologist and Roderick Murchison, an Englishman, set out in the 1830's to study together the rocks which lay between the basement rocks of Scotland and the well defined sedimentary layers of the English coast to the southeast. Their studies centered on Wales. Sedgewick went to the base of the section in the NW where the rocks are highly deformed and sparsely fossiliferous. Murchison worked on the SE near England. In 1835, they proposed two new periods of geologic time. Sedgewick proposed the Cambrian,

from Cambria the Roman name for Wales, and was based solely on lithology and superposition. Murchison proposed the Silurian, named for the ancient tribe that lived in the area, and was based on its fossil content. Since they worked from opposite ends of the section, it seems inevitable that some strata in the middle were claimed by both. We need note in passing that while they were still friends, they together proposed the Devonian period, based on lithologies and fossils, and recognized that different rocks and fossils can be formed at the same time in different places, a profound discovery. We also need to remember that the Cambrian, the Silurian and the Devonian, all survive intact today. But Murchison and Sedgewick were not to be friends for long and they both died waging a classic feud about whether the middle rocks were Cambrian or Silurian. Murchison got the upper hand, because he had described type-fossils for his period. With no faunal basis to draw on, Sedgewick's Cambrian was gradually swallowed up in the Silurian, as others found more and more fossils in his Cambrian. When Murchison became Director of the Geological Survey of Great Britain, Sedgewick was doomed and the entire Cambrian was swallowed up temporarily in the Silurian.

Within a few years after their deaths in the early 1870's, Charles Lapworth, a Scot, etched his name immortally into the geological literature. Lapworth went to the disputed area, carefully studied the evidence in the field and found that the rocks contained an assemblage of fossils that were clearly different than those in either the Cambrian or the Silurian. For these rocks he proposed a new period, the Ordovician, and that name is now universally used. So here we have evidence of embraced theories blocking the mind to new possibilities. It cannot go without notice, that the controversy persisted for some 40 years until the protagonists were no longer capable of defending their views in person.

The revolution that has taken place in the earth sciences since the mid 1960's results from the combination of a few brilliant discoveries, primarily on the ocean floor, and a theory which has been around for at least 340 years. In 1620 Francis Bacon noted that the shorelines of western Africa and eastern South America were remarkably similar and speculated that this was no mere coincidence. Others later suggested that the two continents were once together and had drifted apart. Alfred Wegener, in 1912, published detailed geologic field evidence that all of the continents were at one time together and had subsequently been fragmented. The few supporters of the concept, most notably the South Africans Alexander du Toit in the 1930's and Lester King in the 50's, passionately defended the concept as they gathered more and more field evidence. But there were those, especially in this country and in Europe, who were repulsed by it, argued vehemently against it, and the defenders took on the mantle of carnival freaks. The objections centered on the lack of a mechanism to separate the continents and, as Chuck Drake<sup>3</sup> has said, "the horror of the concept of a continent plowing through the oceans raising mountains as a bow wave and rifts in its wake."

The proponents of the horizontal stability of the earth's crust were not unknowns. Maurice Ewing in a paper in 1952<sup>4</sup> alluded to the ocean basins and said,

"One of the major difficulties in the concept of 'permanence of ocean basins' has been the supposed existence of 'sialic' and 'simatic' ocean basins (Umbgrove, 1947). This obstacle is now removed. The fundamental contrast between the complex crust of the continents and the simple crust underlying the major ocean basins, along with the inconsistencies inherent in theories of continental drift (discussed in the section by Bucher), strongly favors permanent ocean basins as primitive features of the crust dating back to the early history of the earth."



Walter Bucher<sup>5</sup> argued for land bridges with colorful prose.

"The title of this paper suggests the single question to which the following brief discussion attempts to give an answer: Are enough geological and geophysical facts available to make it possible to decide whether or not the concept of continental drift can be used by the student of animal and plant distribution as a working hypothesis with reasonable confidence?"....

"The presence of such transverse welts is certainly incompatible with Wegener's naive, though clever, attempt to explain away what is inconvenient. They show the ocean floor warped into basins framed by swells such as would be expected on an elastic or plastic sheet, but not on the surface of a very weak material showing quasi-hydrostatic behavior."....

"This may well have happened at the right place more than once in the earth's history. But it is very doubtful if it has ever affected really large areas, such as are implied in the traditional ideas concerning the 'foundering' of such hypothetical continents as 'Gondwana land.' Recent studies by Ewing and Press have shown that, contrary to current belief, over large parts of the Atlantic as well as the Pacific and, by inference, the other ocean floors, sialic rock materials are absent. This was proved by combined direct explosion seismic measurements for a small part of the western North Atlantic and analysis of the dispersion of surface waves. The theoretical (mathematical) basis for the traditional view was shown to be erroneous (Ewing and Press, 1950), and, when reinterpreted in the light of the corrected equations and the experience in the western North Atlantic, the records of world-wide earthquakes led to the conclusion as stated.

The student of animal and plant geography must, therefore, get along with assuming either that vast continental areas have foundered to form large areas of the ocean floor, or that continents and islands have drifted in directions he needs for his purposes. There are no short cuts to the understanding of geographical realities."

Bucher was, of course right about sial, but for the wrong reason.

I find it fascinating to re-read, with 30 years of hindsight, the words of Lester King when he answered Ewing, Bucher, and others in his 1953 paper<sup>6</sup> entitled The Necessity for Continental Drift. King began by saying that,

"In the past there has been remarkable cleavage of opinion among geologists on the subject of drift, a large number of Gondwana geologists resident in India, Africa, and South America expressing a profound belief in the hypothesis (e.g., du Toit, Robert, Guimares, Leonardos, Fermor, and Windhausen), whereas a number of equally distinguished Laurasian geologists have held strenuously to the opposite view (e.g., Schuchert, Bailey Willis, and J. W. Gregory).

The basis for such a cleavage of opinion is doubtless to be found largely in the respective environments in which the two groups of savants have worked. Thus Gondwana, being apparently broken into many fragments, making many discontinuities between the respective parts presents geologists there with a set of peculiar and typical problems to which their attention is naturally directed; whereas Laurasia, being in two major pieces only, has not presented observers with a like number of allied problems, and the interest of these, in all but a few cases, has remained purely academic."

He then alluded to the great Karroo basin deposits and stated that their sediments had to extend beyond the boundaries of South Africa.

"Problems of this type are, of course, not peculiar to Africa; North America has likewise its enigmas concerning the derivation of great quantities of sediment apparently from beyond the present boundaries of the continental mass. Americans likewise have explained these phenomena by postulating hypothetical lands of which there are three in particular: Cascadia in the northwest, Appalachia in the east, and Llanoria in the south."

King set out the problem by saying,

"Two modes of deriving such lands have been suggested: one is to elevate the deep ocean floors vertically into new landmasses; the other is to drift already existing landmasses into adjacent positions by horizontal movement. Here is the crux of the matter."

Then he said,

"The mechanics of continental drift, on the other hand, requires two fundamental crustal levels of sial and sima, maintains always the principle of isostasy, and does not involve any such radical physical changes as shortening and lengthening of global radii."

He continued,

"Unlike the vertical elevation and subsidence hypotheses for continents and ocean basins, wherein all convenient gaps in evidence can be presumed to have sunk out of sight beneath the oceanic waters, drift is particularly susceptible of testing. Apposed fragments must fit physiographically, stratigraphically, structurally, and in any other way deemed necessary. The test is critical: *if the apposed fragments do not correspond then the driftist has lost his case; if they do correspond, then the probability of drift being the true explanation becomes immense as the matching details increase in the several categories of evidence.*"

King asked,

"How many geologists realize that the lateral variation between the Table Mountain sandstone of Natal and that of the western Cape Province is notably more than the variation in the same formation between the Cape mountains and the Sierra de la Ventana of the Argentine?"

When many other correlations, as between East Africa and India, are studied the correspondences become truly astonishing,..."

Thirty years ago, King concluded his paper by saying

"The wonderful co-ordinating power of the continental drift hypothesis, harmonizing data in many different categories, is surely apparent. Indeed, we may be amazed that so useful an hypothesis should have been allowed to fall so into neglect or provoked such violent opposition in other quarters. The arguments advanced by some opponents that similar sequences and similar facies (sedimentary, igneous, and metamorphic) in sundered regions such as South Africa and South America are merely fortuitous amounts to a negation of scientific method, for it denies the attempt to classify like data and to generalize from them. It regards the phenomena as random in space and time and denies them any logical significance."

And,

"The physicist and chemist insist that if an experiment is to be accepted it shall be repeatable. In the rocks, that can be seen, sectioned, measured, hammered, and compared, individually and in sequence, structurally and in age, by anyone who cares and has the opportunity to do so, lies the geologist's repeatable experiment. Then becomes apparent the magnitude and detail of the comparisons that are made under the drift hypothesis, the thousands of facts that

fit, under strict control, between apposed continental segments; and conversely, if the comparisons and the facts are to be correctly assessed and understood, of the absolute necessity for thinking in terms of continental drift."

Thus, by carefully studying rocks in the field, King was convinced that drift was the mechanism which had operated in the crust. He recognized this several decades before most geologists in this country, who had to wait for geophysical evidence.

If we move ahead to 1957, and from the Atlantic to southeastern Oklahoma, we come upon a scientific exchange that would be bizarre in any subject. For a number of years in the 20's, the geologists working in the Ouachita Mountains had hypothesized that the mountains were a classic example of flat overthrusting with a root somewhere far to the south. In 1947, T. A. Hendricks<sup>7</sup> of Pan American Petroleum Co. (Tulsa) had elaborated on the concept by describing the western part in terms of a highly complex pile of multiple overthrust sheets. In addition, he believed that the area was first thrust southeast and then back in a northwesterly direction. In 1957, Peter Misch and Kieth Oles,<sup>8</sup> Misch from the University of Washington and Oles with Union Oil Co. (of California), published a note which took this interpretation to task. The title of their note was "INTERPRETATION OF OUACHITA MOUNTAINS OF OKLAHOMA AS AUTOCHTHONOUS FOLDED BELT". I am sure that Misch and Oles came to regret their choice of note as a vehicle for their publication.

They began by saying that

" During a program of extensive geologic mapping between June, 1953, and January, 1956, the writers failed to find any large, flat overthrusts in the Ouachita Mountains. On the contrary, the stratigraphic and structural evidence indicates that the Ouachita Mountains are an autochthonous folded system."

and went on to describe their evidence and its interpretation.

It took Hendricks about 3 months to issue a nine page missive which was called a Discussion.<sup>9</sup> The tone is set in the Introduction.

"The geological note by Peter Misch and Keith F. Oles (1957) published in the August, 1957, issue of this *Bulletin* has certain aspects that can not be permitted to stand in geological literature without comment. In general, these features fall into four groups, as follows.

1. The condemnation of published work and the assertion of the existence of certain geological features without the publication of basic supporting data.
2. The use of incorrect, misleading, and inadequate references to available literature.
3. Presentation of geological data as ostensibly original, although the same data have been reported by earlier workers.
4. The categorical statement of purported geological facts that are contrary to and give no recognition to published data.

Misch and Oles publish no original data that can be reviewed critically. Therefore, discussion is limited to data published elsewhere. Purely factual and other data with only the most elementary interpretive aspects are available in the literature. In preparing this discussion, an effort was made to follow such data closely and avoid theoretical and speculative questions as far as possible."....

"Since the published note is called 'Preliminary Report,' presumably it is not the intent of Misch and Oles to establish validity of their opinions solely by the positiveness of their assertions, but rather that they will ultimately follow the accepted procedure in scientific fields of publishing their data for critical examination by any interested geologists and in a form that will permit study of significant outcrops in the field."

If anything were left out for our purposes, it is in the Conclusions.

"....abundant evidences of low-angle overthrusting not only exist in the Ouachita Mountain structural province but have also been pointed out in publication for observation and further study. If Misch and Oles attempt to revise published data and concepts regarding the structure of the Ouachita Mountains, they have an obligation to present their basic data and to recognize and to present correctly the data and interpretations of previous authors,..."

Fair enough!

The reply by Misch and Oles<sup>10</sup> runs to 19 pages of very fine print, and they must regret having their full paper stuck in the back of a journal under Discussion. But it tells those of us who read it now, most about the technical aspects of the argument. They began,

"Thomas A. Hendricks' Discussion (1958, referred to herein as 'Discussion') of our preliminary 'Geological Note' (1957, referred to here as 'Note') goes somewhat beyond an impersonal discussion of geologic facts and interpretations and calls for an immediate reply, even though the publication of a full report is planned wherein structurally pertinent data, with maps and cross sections, will be presented in detail.

The generalized, and perhaps a trifle sweeping, charges listed in the 'Introduction' to Hendricks' 'Discussion' will not be commented upon in similar generalized terms. Insofar as Hendricks has attempted, in his subsequent chapters, to substantiate these generalized charges by presenting specific cases, these latter will be discussed here. We have examined carefully all of the specific cases presented in the 'Discussion,' and submit that they fail to substantiate any of the broad charges made. We refrain from making any general comments concerning the character of the 'Discussion' and let the facts speak for themselves. To document these facts it is, unfortunately, necessary to go into considerable detail. This is done in Section II for some of the most important specific cases raised in the 'Discussion.' In Section III a general point is presented which is pertinent to any critical evaluation of our short summary 'Note.' In subsequent sections, IV through VII, the remaining specific cases raised in the 'Discussion' are taken up, some of them of necessity somewhat more briefly than is done in Section II, since we do not wish to write a volume. We regret that the character of the 'Discussion' has forced us to take up as much space as we do."

and continued

"In view of this published record, it is somewhat surprising to find that, in his criticism of the statement quoted from our 'Note,' Hendricks says ('Discussion'): 'I know of no publication by a geologist experienced in the Ouachita Mountains who attributes to the Choctaw fault great horizontal movement at its outcrop or considers it a low-angle fault at its outcrop.' We admit that we fail to grasp how an overthrust which is characterized by great horizontal displacement can suddenly lose this character where it happens to intersect the present surface, irrespective of whether it steepens frontally or not."

"Far be it from us to 'condemn,' but we *do* reserve the right to disagree. Moreover, we hope confidently that all of us in the geologic fraternity shall insist on this basic right for ourselves as well as for our critics, and if possible exercise it free of personal involvement."

"...we can not help it if we found an unbroken anticline where a thrust had been mapped, and regular synclines on both sides of this anticline."

The map published by Misch and Oles, Figure 1, is revealing in the contrast between the interpretation of Hendricks and their own.

With respect to the map, Misch and Oles say,

"We can not help it that at a number of places we found intact shale sequences exposed next to mapped large overthrusts and that in general at mapped overthrusts we failed to encounter mechanical rock deformation of the extreme intensity which would be commensurate with the great horizontal translation of major thrust plates envisaged by the allochthonous interpretation."

and

"At the end of the chapter entitled 'Categorical....,' the 'Discussion' takes us to task for not showing, in our index map, some outcrops which, on the scale of this map, have a maximum width of 1/128th inch."

"This point is that we did not take it upon ourselves to 'invade' the Ouachita Mountains with any intent of 'showing anybody to be right or wrong,' having, on the contrary great respect for the body of knowledge accumulated by many distinguished workers in this region."

"If we agree, with the general structural conclusions of some authors and disagree with those of some others, we have no ax to grind. Our only interest is to try to contribute to the search for the truth."

That was the end as far as I am aware. This controversy appeared to be held closely among the protagonists, and having at last seen the evidence of Misch and Oles, Hendricks chose not to rebut.

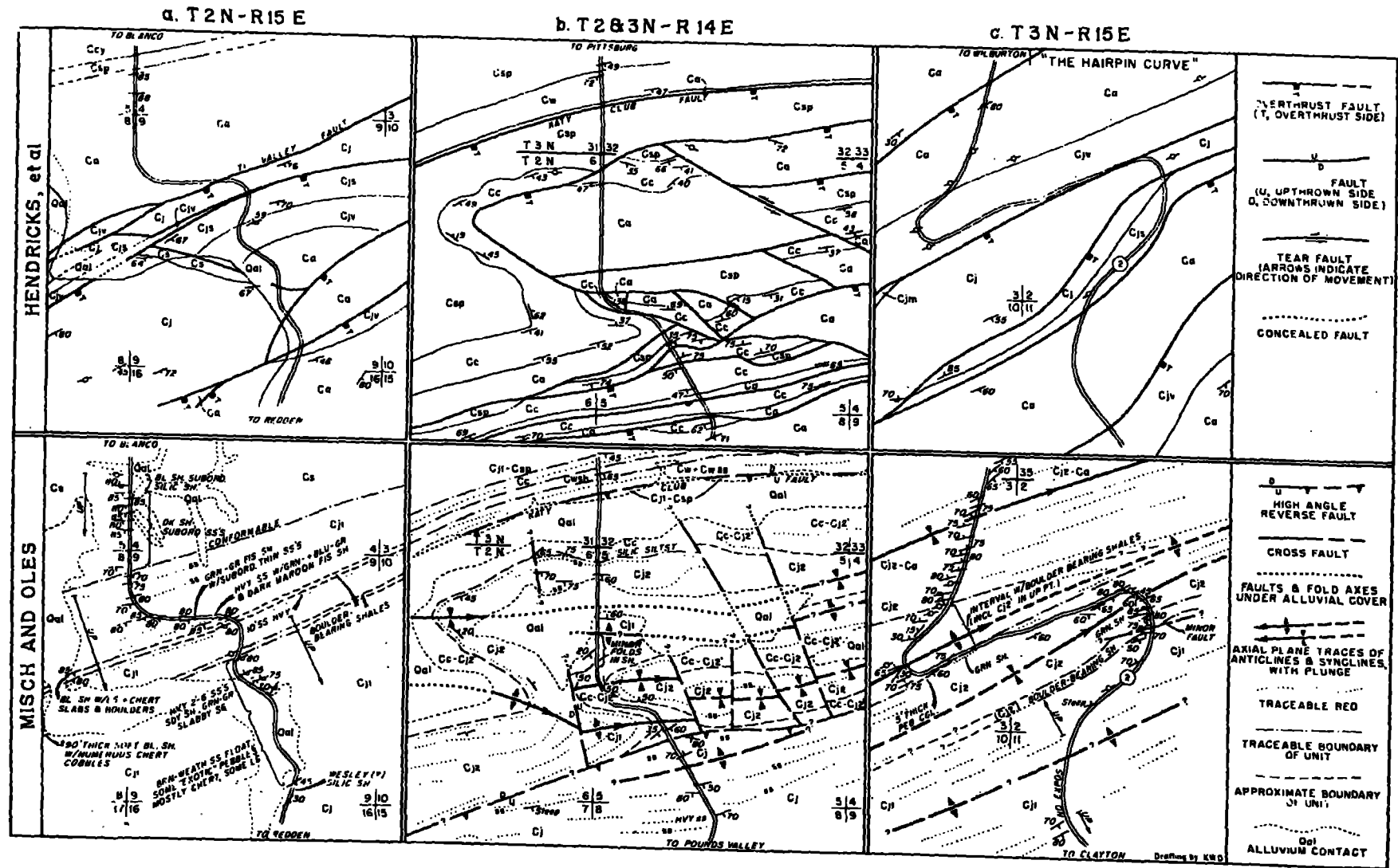


FIGURE 1



The most remarkable controversy, and very poignant in contrast to the almost humorous actions of the protagonists we just saw, is the saga of J. Harlan Bretz. As a young geologist from the University of Chicago, Bretz shocked the geologic community and established himself as an iconoclast with the results of his studies of the Channeled Scablands of eastern Washington, begun in 1922. With painstaking fieldwork, before areal photography, he documented the field relationships. He concluded that there could be no other explanation than a catastrophic flood. The problem was that Bretz called for a catastrophic flood at a time when the prevailing theory was uniformitarianism. This had the effect of immediately sending the uniformitarians to the trenches. The reaction was that "this heresy must be gently but firmly stamped out."<sup>11</sup>

Before reviewing the responses in detail, a few more facts about Bretz's great flood are in order. In a 1923 paper,<sup>12</sup> he calculated that the water

"...was...from 150 and 350 feet deep. Its width averaged at least a mile."

Bretz went on to say,

"Fully 3,000 square miles of the Columbia plateau were swept by the glacial flood, and the loess and silt cover removed. More than 2,000 square miles of this area were left as bare, eroded, rock-cut channel floors, now the scablands, and nearly 1,000 square miles carry gravel deposits derived from the eroded basalt. It was a debacle which swept the Columbia Plateau."

He then said,

"Computations such as these strongly incline one to doubt the actual occurrence of the flood. The writer has repeatedly been driven to this position of doubt, only to be forced by reconsideration of the field evidence to use again the conception of enormous volume. It is the only adequate explanation of the phenomena. These remarkable records of running water on the Columbia Plateau and in the valleys of Snake and Columbia rivers cannot be interpreted in terms of ordinary river action and ordinary valley development."

Bretz<sup>13</sup> calculated flow rates at about 40 cubic miles per day (66 million cubic feet per second.). Velocities attained were as high as 30 m/s. To be supplied by melting ice would have required the conversion of 42 cubic miles of ice per day. When he later<sup>14</sup> found the source for his flood, the glacial Lake Missoula, he postulated that the catastrophic failure of an ice dam would have released 500 cubic miles of water. He<sup>13</sup> had found a 200 square mile delta in the Willamette Valley of Oregon, the result of his flood.

In 1927 Bretz met with a phalanx of doubters in Washington, D. C. For those who have not studied the history of this debate, it is necessary to emphasize that unlike King or Misch and Oles, he was virtually alone. At that meeting Bretz presented the basis for his ideas. Some insight into the passions that accompanied that meeting can be obtained by examining the record.

W.C. Alden<sup>15</sup> said,

"Professor Bretz frankly points out the difficulties met in applying his explanation of the origin of the remarkable features of the Columbia plateau. It is not easy to one, like myself, who has never examined this plateau to supply offhand an alternative explanation of the phenomena. I have read Professor Bretz's papers on the subject with great interest but I am left with the feeling that some things essential to the true explanation of the phenomena have not yet been found. The 'channels' appear to be due to stream erosion. The main difficulties seem to be: (1) The idea that all the channels must have been developed simultaneously in a very short time; and (2) The tremendous amount of water that he postulates as coming from the melting of the ice sheet in so short a time to do the work."

James Gilluly,<sup>16</sup> who was a domineering figure in American geology in this century, said,

"The question of the existence of a Spokane flood rests on the interpretation of many highly abnormal field facts. The evidence presented by Professor Bretz is assuredly convincing as to (1) the anomalous, indeed

unique, drainage features of the Columbia Plateau, (2) their direct dependence upon glacial waters, and (3) the necessarily large volume of many of these streams. However, certain criteria used to determine the actual quantities of water involved appear somewhat questionable."

and concluded with

"That the actual floods involved at any given time were of the order of magnitude of the present Columbia's, or at most a few times as large, seems by no means excluded by any evidence as yet presented."

Two footnotes are worth mentioning here. Gilluly had not studied the scablands in the field and Bretz had in fact been his high school teacher at Gilluly's in Seattle. Gilluly concluded that Ockham's razor did not apply and devised a more complex scheme calling for adjustments by various floods and rivers.

Although no-one argued with Bretz's carefully documented field evidence, they did argue with his interpretation.

"The hypothesis of a single tremendous flood should not be accepted without further detailed regional study."<sup>17</sup>

and

"I have seen only the part of the region under discussion, that including Quincy Valley, Grand Coulee, and Moses Coulee. As Doctor Bretz has stated, the erosion features of the region are so large and bizarre that they defy description. However, the Columbia River is a very large stream, especially in its flood stages, and it was doubtless still larger in the Pleistocene epoch."<sup>18</sup>

Bretz<sup>19</sup> answered in detail and made his position quite clear.

"I think I am as eager as anyone to find an explanation for the Channel Scabland of the Columbia Plateau which fits all the facts and will satisfy geologists. I have put forth the flood hypothesis only after much hesitation and only when accumulating data seemed to offer no alternative."

Bretz then returned to the field and traced the scabland tributaries up their channels as he mapped flood deposits. He found evidence of the flood as far upstream at 100 miles into Idaho. He then wrote,

"The writer, forced by the field evidence to this hypothesis, though warned times without number that he will not be believed, must call for an unparalleled rapidity in the rise of the scabland rivers."<sup>20</sup>

In a 1928 paper<sup>21</sup> he said,

"Ideas without precedent are generally looked on with disfavor and men are shocked if their conceptions of an orderly world are challenged. A hypothesis earnestly defended begets emotional reaction which may cloud the protagonist's view, but if such hypotheses outrage prevailing modes of thought the view of antagonists may also become fogged.

On the other hand, geology is plagued with extravagant ideas which spring from faulty observation and misinterpretation. They are worse than 'outrageous hypotheses,' for they lead nowhere. The writer's Spokane Flood hypothesis may belong to the latter class, but it can not be placed there unless errors of observation and direct inference are demonstrated. The writer insists that until then it should not be judged by the principles applicable to valley formation, for the scabland phenomena are the product of river channel mechanics. If this is in error, inherent disharmonies should establish the fact, and without adequate acquaintance with the region, this is the logical field for critics."

For the next 20 years he was villified and a plethora of hypotheses were advanced to explain his field observations, many by such notables as the glacial geologist R. F. Flint. Gradually, evidence of ripple marks 15 meters high and with a wavelength of 150 meters, in Lake Missoula and other similar data downstream convinced the critics that Bretz was correct. The end of the controversy perhaps came when in 1965 an international party of geologists visited the area and sent Bretz a telegram which began "Greetings and Salutations!" and ended "We are all now catastrophists."<sup>22</sup>

There is something sweet in the fact that in the 1970's, when the Mariner and the Viking missions reported on the Martian surface, they indicated that there had been extensive scouring and channeling. Bretz's catastrophic

flood hypothesis was then invoked in explanation. The sweetness is in Bretz's having lived to see those results. For more detail about the trial of J. Harlan Bretz I refer the reader to the documentation of Victor Baker.<sup>23,24</sup>

There are of course many controversies in the geological sciences. The preceding examples are indicative general trends. What conclusions can we draw?

I have said that the dispute over tektites is typical of other disputes in the geologic sciences. Yet in a large sense it is not. Everyone these days is entirely too nice. This is probably a reflection of too much hard data which causes everyone to pause and think, and the presence of government funding agencies which might be forced to choose sides in a controversy, at least in terms of funding. Nevertheless, one has the feeling that the elements of a good dispute are indeed there and that the solution to the problem involves observations that are not now part of the dogma. Perhaps some feel like Bretz, or King, or maybe even Tom Hendricks.

What do these controversies tell us? First I think, to contemplate ones arguments in terms of other views. Consider what might have been different had Hendricks thought about what Misch and Oles had really said; or if Gilluly had listened more carefully to Bretz's admonitions that he had considered other and more potentially desirable explanations; or if any number of people had not felt that Lester King had limited his horizons to the Southern Hemisphere. While the early arguments for drift were not complete, the rebuttals, in retrospect, seem woefully inadequate.

Second, be sure to make careful field observations. After all, the evidence of natural phenomena is in the field. Some of the most profound discoveries and achievements in the geological sciences since World War II have been made by physicists and chemists who became deeply interested in natural phenomena and took themselves to the field. Louis Agassiz, whose glacial theory provided much of the basis for uniformitarianism said, "Study nature, not books!"<sup>25</sup>

Third, search for outrageous hypotheses. A president of the Geological Society of America once said,

"...How narrowly limited is the special field, either in subject or locality, upon which a member of the Geological Society of America now ventures to address his colleagues....I wonder sometimes if younger men do not find our meeting rather demure, not to say a trifle dull; and whether they would not enjoy a return to the livelier manners of earlier times....[Their] feeling of discouragement must often be shared by the chairman of a meeting when, after his encouraging invitation, 'This interesting paper is now open for discussion,' only silence follows....We shall be indeed fortunate if geology is so marvelously enlarged in the next thirty years as physics has been in the last thirty. But to make such progress, violence must be done to many of our accepted principles."

That statement was made in 1925 by W.M. Davis.<sup>26</sup>

Finally, look for the simplest explanation. Ockham's Razor does seem to apply. It is all too easy to present complicated theories that no one can defend.

## REFERENCES

1. Watson, J. D., The Double Helix, Atheneum, New York, 1968.
2. Chamberlin, T. C., J. Geol. 5, 837-848 (1897).
3. Drake, C. L., Condon Lectures, Oregon State System of Higher Education, Eugene, Oregon (1970).
4. Ewing, M., in The Problem of Land Connections Across the South Atlantic, with Special Reference to the Mesozoic; E. Mayr, Ed., Am. Museum Nat. Hist. Bull. 99, 87-91 (1952).
5. Bucher, W. H., in The Problem of Land Connections Across the South Atlantic, with Special Reference to the Mesozoic; E. Mayr, Ed., Am. Museum Nat. Hist. Bull. 99, 93-103 (1952).
6. King, L. C., Bull. Am. Assoc. Petroleum Geologists 37, 2163-2177 (1953).
7. Hendricks, T. A., et al., U.S. Geol. Survey Prelim. Map 66, Oil and Gas Investigations Series (1947).
8. Misch, P. and K. F. Oles, Bull. Am. Assoc. Petroleum Geologists 41, 1899-1905 (1957).
9. Hendricks, T. A., Bull. Am. Assoc. Petroleum Geologists 41, 2757-2765 (1958).
10. Misch, P. and K. F. Oles, ibid. 41, 2765-2783 (1958).
11. Bretz, J. H., H. T. U. Smith, and G. E. Neff, Geol. Soc. Am. Bull. 67, 957 (1956).
12. Bretz, J. H., ibid. 34, 573 (1923).
13. Bretz, J. H., J. Geol. 33, 97 (1925).
14. Bretz, J. H., Geol. Soc. Am. Bull. 41, 92 (1930).
15. Alden, W. C., J. Wash. Acad. Sci. 17, 200 (1927).
16. Gilluly, J., J. Wash. Acad. Sci. 17, 203 (1927).

17. Mansfield, G. R., J. Wash. Acad. Sci. 17, 207 (1927).
18. Meinzer, O. E., J. Wash. Acad. Sci. 17, 207 (1927).
19. Bretz, J. R., J. Geol. 35, 461 (1927).
20. Bretz, J. R., ibid. 37, 393 (1929).
21. Bretz, J. R., Geol. Soc. Am. Bull. 39, 643 (1928).
22. Bretz, J. R., J. Geol. 77, 505 (1969).
23. Baker, V. R., Science 202, 1249 (1978).
24. Baker, V. R., ed., Benchmark Papers in Geology / 55, Dowden, Hutchinson, and Ross, Stroudsburg Pa. (1981).
25. Ref. 23.
26. Davis, W. M., Science 63, 463 (1926).

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